SCIENCE

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A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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SATURDAY MAY 21, 1881.

THE Spring Reception of the American Museum of Natural History, Central Park, New York, and the publication of the Twelfth Annual Report, remind us of the existence of this Institution, and recall its many claims for support from those interested in science, and in the intellectual progress of the people.

The additions and improvements made during the past year make only a short list on the programme, but reflect the excellent management and zeal of the officers in charge of the collections. A large portion of the collections of birds and mammals has been remounted on newly designed stands, the results obtained being most creditable to those who have carried out this improvement.

One of the new features of usefulness recently established is an Economic Department, which will contain specimens illustrating the Economic Botany of all the woods of our country, that are to be used for architectural or building purposes, or in the manufactures, each species being fully represented by specimens of the leaf, flower and fruit.

We fully appreciate the exertions made by the Trustees of this Institution to extend the usefulness of the Museum and to make it a means of teaching the laboring classes the value of scientific knowledge, and its practical bearing on many of the industrial pursuits of life. If such is, at least, one of the objects of establishing this Museum, it is difficult to understand the action of the Trustees in closing its doors on Sundays, that being the only day on which the artisan and mechanic can make a visit, without entailing a direct loss on himself and family. A petition, signed by 16,000 citizens of New York, was recently offered as a direct appeal to the Trustees to accord this privilege to the working classes, and we trust that the board of management, which has always shown a most liberal and enlightened spirit of enterprise in the conduct of this Institution, may reconsider its late decision on this subject.

The city of New York has provided a costly building for the Museum and recently appropriated $35,000 to improve the approaches. In the Report of the Trustees now before us, a direct appeal is made to the people for financial aid and support. We, therefore, believe the Trustees would confer a direct benefit on the Institution by opening its doors to the people on Sundays; the Museum would doubtless become one of the most popular Institutions in the city, and the Legislature would probably respond with no grudging hand, to provide means for the completion of the building and for its maintenance on a liberal scale.

THE announcement is made of an improved method of storing electricity, by M. Camille Faure, of Paris, the London *Times* asserting that "a box of electric energy nearly equivalent to a million feet, contained within less than a cubic foot of space, intact and potential, has been transported from France to Great Britan."

Sir William Thomson is said to have given some endorsement to the discovery, and tests and measurements are in progress at the laboratory of the Glasgow University.

The principle involved in M. Faure's discovery is understood in this country, and the possibility of its general correctness is conceded. The language employed in the announcement is rather equivocal, and the misuse of scientific terms render the exact extent of M. Faure's discovery a matter of some doubt.

We gladly welcome any progress in electical science; but as the necessity for storing electrical energy is of value only in very rare cases, the practical usefulness of M. Faure's discovery must be limited in extent.

MR. EDISON has courteously responded to a request on our part, to offer his opinion on M. Faure's discovery, and we take pleasure in placing before the readers of "SCIENCE" his reply, received since our own notes were in type.

To the Editor of "Science."

DEAR SIR: The Faure battery is an improvement on the Planté battery.

Planté was, I think, the original inventor of the battery which bears his name, invented some years ago for the purpose of storing up electricity.

Faure has simply made a Planté battery, by some means reducing its resistance, and thus reducing the percentage of loss. This is all there is in it.

Some two years ago I patented and applied a method for using the Planté battery in connection with electric lighting. Yours truly,

THOS. A. EDISON.

SENSIBILITY AND ITS DIVERSE FORMS.

By M. Oltramare.

[Translated From the French, by the Marchioness Clara Lanza.]

From the feeble cry by which the infant affirms simultaneously its birth and its sensibility, to the last long drawn sigh which bids adieu to existence, human life oscillates constantly between two opposite conditions created by the nervous system—pleasure and pain, joy and sorrow.

Being creatures developed to a great extent under the influence of the senses, we experience to an extreme degree, the action of all exterior agents, and atone for such pleasures as are granted to us by our exquisite sensibility, with moral and physical suffering. Not satisfied with momentary impressions, we foresee the influences which are to reach us, and by means of our refined intelligence we create those two great incentives to our actions—apprehension and desire.

Being mortal and also conscious of the fact, we naturally have a presentiment of the final destruction of our bodies, and most of us fear this and look upon it with dread. Nothing of this kind, however, is to be met with in animals. The last hour of life, brutal and violent though it may be and totally unexpected, does not affect them. The dog licks his master's hand affectionately whether it be extended to caress or to kill. He is no more conscious of the possibility of death, than is the ox which is led to the slaughter house.

These higher animals have nevertheless, a sensiblity and individuality upon which their reason depends. They possess what we call instinct. But, as we descend further in the animal world, we see that this function gradually diminishes in proportion as the organisms become simplified, until finally we reach a point, where to cut a living creature in two, not only produces no perception of pain, but actually becomes a means of reproduction, each half being capable of forming a distinct organism precisely like the original.

Lower still, we come to plants, which are living organisms, although Linnæus, a naturalist of the highest rank, refused to admit their sensibility. He says: "Plants live and grow; animals live, grow and feel."

This theory recalls that of Aristotle, when the Grecian philosopher affirmed that all organized beings had a soul more or less developed.

To the vegetable soul he attributed two faculties—growth and reproduction. To the animal soul he assigned four faculties—growth, reproduction, sensibility and motion. To the human soul, five faculties. The four above mentioned, to which was added intelligence, or mind.

Neither Linnæus nor Aristotle admit of any sensibility in vegetable life, and yet this is as great an error as to deny the existence of this same faculty in animals. An error, which is almost universal even in the thinking world, and which certainly should no longer be allowed to exist. From the most minute plant, to the most perfect animal, we find sensibility under various forms, but always corresponding to Claude Bernard's definition: "Sensibility is the *ensemble* of all kinds of modifications, determined in living things by different stimuli, or rather, the aptitude to reply to the provocation of these stimuli by means of modifications."

Bichat distinguishes three forms of sensibility:

1. Conscious sensibility, which presides over relations to exterior movements.

2. Unconscious sensibility, representing internal movements.

3. Insensible, or imperceptible sensibility, so called because it is manifested in other ways than by movement.

Putting aside these fine distinctions, let us admit two forms of sensibility—conscious and unconscious—and we shall be able to demonstrate the possibility of a passage from one state to the other, which proves that they are but modifications of a single attribute.

When we learn to read, it is with considerable difficulty, and we doubt if any one ever mastered the art unconsciously. But later, can we not peruse page after page mechanically, without having an idea of their contents? A transformation has therefore taken place in two kinds of sensibility. It is precisely the same with walking and many other acts in which the brain—that is to say, the conscious agent—plays but a secondary part.

If I prick the foot of a frog with a needle the animal draws it away, and, forewarned by the pain, endeavors to escape. Sensibility here evidently assumes a conscious form. If, however, I decapitate the frog, that is, if I destroy the organ which is the *ego*, so to speak, and once more perform my experiment, the mutilated body draws the leg away, but makes no attempt to escape. The act is purely reflex, unconscious, and in this case, by a simple experimental artifice, I am able at once to substitute the second form of sensibility for the first.

We breathe without knowing it, without the intervention of our will; but if our attention is directed upon this mechanical act, we become immediately conscious of it.

In eating, when once our food is swallowed we know nothing more about it, and yet our sensibility is constantly played upon by these substances, which, physically and chemically modified, are introduced into the circulation of the blood, and thence carried to the anatomical elements, whose sensibility they incite to action. All vital properties, and, consequently, sensibility, reside in those little numberless organic unities which go to form living beings.

There exists a fundamental matter, protoplasm, an amorphous substance endowed with peculiar properties and which Huxley has justly termed the physical basis of life. This protoplasm, which sometimes alone constitutes an inferior living creature, not only moves but attaches to itself minute particles which it meets with in the water, digests them and assimilates them with itself. Ether, the great reagent of sensibility, causes it to lose its transparency, and its movements to disappear. Then, when it is evaporated, the fluid reappears with all the attributes of this inferior life. This is undoubtedly sensibility, but in an unconscious form.

If we begin to mount the organic ladder, we see gradually appear certain cells which specify sensibility, and which, created solely to perform this function, elevate and perfect it. These are the so-called nerve cells. They are scattered throughout living organisms; in the higher animals they are very numerous, and serve to centralize impressions and produce individuality. When they are united to others called cephalic cells, they admit not only of sensation, but also the interpretation of sensation which then becomes conscious.

Thus, beginning with this infinite attribute of living matter which Haller and Glisson, being too timid to call sensibility, termed irritability, we gradually come to the highest forms, whence originate the greater portion of intellectual and physiological phenomena.

In man, all the sensible nerve cells are united in one mass called the cerebro-spinal axis, or the encephalo-medullary mass. It is composed of the spinal cord, the medulla oblongata, and the brain, each of its departments representing one form of sensibility. The spinal cord, properly speaking, corresponds to unconscious sensibility. This is illustrated by that involuntary and spontaneous movement which we call reflex action. The medulla oblongata controls sensations which, like respiration for instance, are frequently unconscious, but which, however, by an effort of the will, can be interpreted as precisely the opposite. The brain possesses the highest form of sensibility, and it is here that the greater part of our physical and intellectual acts are performed. By means of the mi-

croscope we are able to-day, to separate in each nervous centre, the sensitive cells from others of a like kind performing different functions which can be recognized by their shape, dimension and situation.

It is useless here to go into minute details concerning this point. I will call attention, however, to the fact that each sensitive nerve cell is connected with exterior agents by a long fibre called the cylinder axis, which resembles a telegraph wire carefully concealed by a layer of fat, and which, surrounded by numerous protecting membranes, extends throughout portions of the body, and produces sensibility. All these nerve fibres, whose receptive apparatus is in the encephalo-medullary mass, are grouped together and form those little white filiments which we designate as nerves. If the end of a nerve is touched, or the root, a modification can instantly be determined, which carried to the nervous centres, becomes a sensation. This sensation is, of course, not always the same, but is in accordance with the determining agent, optic, acoustic, gustative, etc.

If for instance, we cut the nerve which conducts light from the eye to the brain, this sensation will immediately be felt; but if, on the contrary, one of the skin nerves be cut, intense pain will be experienced. It is not, therefore, as M. Delboeuf very justly remarks, the nature of the excitation which determines that of the impression, but the manner in which the brain centre is brought into activity; so that if the optic and acoustic nerves be cut, united and inverted, a noise would be interpreted by a sensation of light, and *vice versa*. The sight of a picture would determine sounds in relation to the brilliancy of the paint employed, while an orchestra would produce colors varying, according to the sounds. Sensations experienced in consequence of exterior impressions do not therefore depend upon the character of the latter, but upon the nature of our nervous cells. We do not feel that which occurs upon our body, but only that which takes place in our brain. If, therefore, all our nerve cells were identical, the exterior world would doubtless produce sensations, but they would be precisely alike, merely differing in intensity. There are certain animals which exist in this condition.

M. Helmholtz and other physiologists have calculated the amount of time required for the transmission of the excitation to the sensitive nerves, and have decided upon thirty metres a second—that is to say, a rapidity equal to an express train advancing at full steam power. Imagine a man whose brain is in Paris while the extremity of one of his limbs is in Geneva, and we will see that it must require precisely four hours and forty-four minutes for a sensation to pass from the latter city to the former.

Given the small distance which separates our extremities from the nerve centres, and the time of transmission is short. It is remarkable, however, that those organs which play the greatest *rôle* in the preservation and conservation of the individual, sight and hearing should be placed in close proximity to the brain. This produces a rapid transmission, and enables a speedy evasion of destructive objects—a disposition evidently acquired by natural selection. It seems, moreover, that the intensity of the impression is in accordance with the distance intervening between the excitation and the nerve centre. We may thus explain the extreme violence of neuralgia of the face and head, as compared with that affecting other portions of the body.

All the various forms of sensibility have an analogous basis. The connection and fundamental identity can be demonstrated by the action of narcotics. We shall see that this is the most general and characteristic property of life, and this axiom can be fully established—that everything which lives, whether animal or vegetable, feels and can be rendered insensible.

It is a well known fact that certain plants rebound when they are touched. The sensitive one closes its leaves while a great number of carnivorous plants shut up like traps as soon as a fly alights upon them, imprisoning and crushing the poor insect which is to serve them as nourishment. The action of day and night has been equally verified in regard to plants. Some flowers only open when the sun shines, while others bloom solely in the dark. It has also been seen that the leaves sometimes turn towards the sun, but these phenomena have been called exceptional, many persons even placing them in the category of problematic occurrences, not wishing evidently to open their eyes to facts which they consider humiliating to the animal species.

Now, however, doubt is no longer possible. Ignorance upon this point can no longer be permitted, and every one must know that animals and plants alike possess sensibility. A great philosopher and physiologist, Claude Bernard, first demonstrated this important truth, not by means of tortuous reasoning, but by the brilliant light of experiment.

Provided with an anæsthetic agent, ether or chloroform, he was able to prove that the highest forms of conscious sensibility and the lowest forms of unconscious sensibility can be successfully affected. When the action of the narcotic begins to take place the *ego* sleeps and with it, conscious sensibility. That is sufficient for the surgeon who can then begin to cut and burn without the shadow of an *arrière pensée*.

Upon continuing the introduction of the fumes of ether into the organism, we see all the forms of unconscious sensibility gradually become annulled subsequent to conscious sensibility. After having acted upon the nerve cell, the anæsthetic destroys the sensibility of all the tissues, that is to say their vital characteristic, causing them to react upon exterior agents—in one word, it kills the individual.

If we pass on from the animal to the plant, we find that ether and chloroform act in identically the same manner. Subject the leaves of a sensitive plant to the fumes of either of these agents, and you will be able to handle them without eliciting the slightest movement on their part. They no longer feel the contact of the hand, for knowing as we do that anæsthetics respect the functions of movement, we can only attribute this inertia to the impotency of the excitation.

Let us now take a rapidly germinating seed, such as that of the water-cress and place it upon a sponge soaked in water. In twenty-four hours it will have blossomed into a tiny stem and root. Repeat the experiment under the same conditions of oxygen, water, light and heat, but place the sponge beneath a glass which has been dipped in ether. The seed will remain intact. It is not dead however, it merely sleeps, for if we remove the glass it will recover from its stupor and by the following day will have sprouted. This unseen life possessed by the seed, life which asks nothing more than to make itself apparent, is, however, subject to external and internal conditions. The first are the necessity of water, oxygen, heat and all physio-chemical conditions; but there is still something else, internal, inherent to the seed itself and constituting the essence of its life. It is sensibility. Destroy this function and notwithstanding the most favorable surroundings, the development will be effectually stopped.

Do not think that this is owing to any peculiarity of the plant and its embryonic condition, for a hen's egg, that latent condition of life of an organism belonging to a comparatively high order, cannot be hatched with any desirable result in an etherized atmosphere.

Germination, the first vital act of the individual, be it plant or animal, is therefore subject to sensibility and in this function we see it appear for the first time. Afterwards it is not difficult to follow it in its course through all the vital acts of the organism. The plant breathes and grows by assimilation, absorbing either the substances contained in the earth, or the carbonic acid in the air. For a long time this gaseous assimilation was

confounded with respiration, and the mistaken conception was spread abroad that plants breathe in direct opposition to animals by absorbing carbonic acid and exhaling oxygen. By means of anæsthetics we can separate these two phenomena. An aquatic plant placed in etherized water ceases to absorb carbonic acid and emit oxygen. It however, remains green, and breathes as animals do, a phenomenon which existed before, but was hidden by the assimilation of the carbon; still, further back, we can encounter one of those phenomena long considered chemical and which nearly escape vital acts inasmuch as in the laboratory some of them can be reproduced without the aid of life. I speak of fermentations. These are produced by a microscopic fungus, which decomposes fermentable matter, nourishing itself with a portion, while the remainder forms a new product which stays in the liquid. These fermentations, in spite of their extreme tenuity and their inferiority in the organic scale, are susceptible of being stupefied by ether and losing their active power. We may place them with impunity in close contact with the liquid, but the latter remains undisturbed.

Thus, from the very bottom of the ladder, from the simplest protoplasm, and the most insignificant fermentation to the most elevated creature to be found on the earth, we find always the same characteristic and fundamental property of life, modified, it is true, to a degree which forces us to follow the thread of its diverse forms step by step, but always identical in substance, and invariably demonstrable by those infallible reactive agents, anæsthetics. Without this property there can be no life, or rather no active life, no exterior manifestations. With it, any plant or animal, no matter how simple in construction, develops, grows, prospers and reproduces itself. It is easy to see, therefore, that sensibility is the principal attribute of all organic beings, and in some way the cause of everything that takes place within us. If, as Condillac says, we should take an immovable and insensible image and endow it gradually with all our senses, it would soon rise from nonentity and begin to augment the sphere of its knowledge. By giving it the sense of hearing, we open that vast field of observation and reasoning which procures sound, but it could form no idea of the existence of matter, or of sunshine, or of taste. It could only conceive one thing, until put in complete possession of the other senses.

Intelligence, that precious gift which alone renders us superior to other creatures, is, therefore, nothing more than the result of our accumulated impressions, controlled one by the other, and we may even affirm that the man who has felt is alone capable of thought. The development of our minds should be adequate to the development of our sensibility, and in fact, it can be observed everywhere, that those persons whose senses are the most refined, possess the highest form of intelligence. I may even go so far as to parody the famous proverb and say to my neighbor; "Tell me what you feel, and I will tell you what you think."

Not so very long ago, as we have seen, Linnæus refused to admit of sensibility in regard to plants, saying that it was an attribute of the animal world only. An attentive investigation, however, causes us to reject such distinctions to-day. Let us even go further back, leaving behind us the lowest forms of organic matter, and see if any phenomenon approaching sensibility is to be met with. In a word, let us ask the following question: Is matter sensible?

Referring once more to Claude Bernard's definition of the term, "sensibility is the *ensemble* of all kinds of modifications determined in living beings by different stimuli," we find no possibility of its application to the properties of matter, for it distinctly states that the condition is an attribute of living beings only. But a mere definition should not arrest our investigation, for it is nothing more than the result of knowledge hitherto acquired, and as such admits of change. The substance of it all amounts to this; given a living being placed in immediate contact with matter, and the matter will act upon the being, producing sensation. But how do we know that the living being does not in its turn act upon the matter and modify its condition? I will even affirm that life does act upon certain substances, for fermentation is a positive proof that this is the case. If I place a sweetened solution of wine in contact with the air, a short time will suffice to develop therein millions of tiny living creatures proceeding from atmospheric germs. This fermentation increases with great rapidity, producing a chemical effect, so that after a certain time the sugar will be transformed into carbonic acid and alcohol. The presence, therefore, of life in the liquid served to modify the properties, and in this we see one of those strange occurrences where the so-called vital forces are so closely allied to chemical processes, that we hardly know whether the phenomenon is the result of the biologists' skill or the chemist's. Each of these *savants* have claimed it as their own, and with reason too, for chemistry and biology are twin sisters who can never quarrel.

When the sugar is once transformed into alcohol another organism appears, which, in its turn, determines the transformation of this substance into another, acetic acid, by means of an analogous fermentation. It is a remarkable fact, however, that while the chemist has, as yet, been unable to produce alcoholic fermentation by means of the action of matter upon matter, he can, on the contrary, easily determine the second without the aid of life at all. It is, therefore, the presence of these bodies which acts, and not the construction of the fermentation. It is not that life decomposes the liquid, but that the liquid decomposes itself when assimilated with certain agents. It is therefore sensible of their action.

Once *en route*, it is not difficult to multiply examples and to demonstrate that light, heat, electricity and all other forces which operate upon our sensibility, are uniform modifiers of matter. What is a photographer's negative but a glass plate sensitive to the action of light? Is not a piece of wire about which we pass an electric current sensible of electricity, inasmuch as it acquires thereby a new property, that of attracting a like piece of wire? It becomes, in fact, magnetic.

Heat, as we can observe every day, modifies bodies to such an extent, that beneath its influence they liquify and evaporate. All these facts demonstrate clearly that matter is sensible of exterior agents. According to the second part of Claude Bernard's definition, it possesses the "aptitude to reply to the provocation of these stimuli by means of modifications."

Consequently, universal attraction, that law which affirms that all bodies attract each other in direct ratio to their mass, and in inverse ratio to the square of their distance, is merely a simple and general way of expressing the sensibility of matter.

CONTRIBUTION TOWARD A NEW COSMIC HYPOTHESIS.

By Samuel J. Wallace.

Our familiar knowledge and ideas in astronomy relate generally to matter in large bodies, and in great numbers of small bodies, which now and then fall into the larger as meteorites. This seems to show a condition of slow centralization, as if to finally collect all matter, however far distributed, into a few large bodies. And a consistent conception requires in its plan, somewhere, a means of decentralization, or distribution of matter through space again, to form a closed system of action.

Gravic force as one of the interchangeable forms of kin-

etic energy also requires a starting place; some way by which the energy continually changed into mass motion and heat of bodies gravitating into each other shall be changed back again into gravic force.

I think the luminously hot gaseous nebula shows the earlier stages of these two required dispersions taking place together.

The fall of matter from space into central masses takes up just so much gravic energy. This is changed into mass motion and heat, so that on mechanical principles, each particle bears just enough force still to carry it back into space again.

This leads us up to the idea of the life of a particle in the universe, as being through an endless series of cycles of change from space into masses and from masses into space, each being, as it were, almost an eternity in duration, making a grand orbit, something like this:

Meteoric matter falls from space into planets and suns; planets fall into suns; suns grow continuously larger, accumulating the momentum of particles acquired in falling from space, as motion and heat; the heat of overgrown suns at last becomes so intense, from accretions and collisions, that it flies into a still higher form of repulsive energy, as gravic force, causing disruptive explosions; the particles, by this change into gravic force outward, receive a projectile force carrying them out into space again; and particles from space fall again into planets and suns.

This forms a closed cycle of action. The projectile and outward gravic forces may carry part of the particles forward into space till they become parts of other systems to run like courses; and part may be driven back at last to rebuild another system instead of that destroyed. Some might be driven back by the extra-force given others, and form nuclei of one or more suns. And the disrupted parts going in different directions might form a number of nuclei, having such dispersive motion as to carry them continuously apart, as in some reported cases.

The history of a nucleus having such an initial projection might be something like this:

I. It might pursue an interminable course; should this lead laterally near any region having matter in great quantity, it would be deflected to pursue a great curve, attracting to itself distributed matter within its reach, from gravitation.

II. The growth of a body pursuing a curved course would produce in it a rotary motion, which would be the resultant of the course and force of its own prior parts and those of its continued accretions, and of the combined attractions on its several parts of all matter passed, which of course would predominate on the inner side of its curved path.

III. Matter drawn into a moving body would come in curved lines and spiral courses, tending to form great rings and to produce planets, whose courses and velocities would be the resultants of the total averages of those of the various particles uniting to form each of them.

IV. Secondary bodies of large size would approximate primaries in the nature of their actions, falling in by slow degrees; those of long life as secondaries tending to eventual motion in one general direction, from having similar producing forces and from their actions on each other neutralizing conflicting equivalents; giving to each such position as its resultant velocity leads to, by union or separate course, together with revolution, and possibly secondaries to them.

V. Differences of inclination of orbits of secondaries may occur from the course of primaries having been at some time near regions having predominant quantities of matter to be drawn in on special sides, which unites to form bodies having their special courses; and, from analogous causes some smaller bodies might chance to have courses contrary to others.

VI. The four terms of orbital and rotary velocity, heat, and loss from radiance and from a resisting medium, would form a sum equal nearly to the energy of fall from infinity to the mean position of each body, qualified by whatever initial velocity and direction each particle may have had from its prior projections.

VII. The distance of each body from its primary would depend on its velocity in proportion to that which fall from infinity would produce; and all causes changing orbital into rotary motion, into heat and radiance, and into friction on a resisting medium would shorten the mean distance; but add to the velocity.

These I think are about the things which would occur, stamping the history of the system upon its internal peculiarities.

Now, does this sort of hypothesis come nearer accounting for the solar system than the original or present day forms of the "nebular hypothesis?"

I think that requires far too extravagant a conception of a common initial force taking possession of the matter thinly distributed through so immense a region as a globular space extending far out beyond the orbit of Neptune; and that it does not account for the variations of detail, nor for a final closed system of action, as does this.

I have no faith in this stuff about burnt out worlds, thinking that planets and suns grow warmer instead of colder as they slowly grow larger from fall of meteorites, as now going on.

Gravic force, which causes gravitation of bodies toward each other, probably has some resistance and loss in passing through large bodies; which would heat them and give ample source for all extra heat in the sun and large planets, and the interior of the earth; and would give ample time to geology for all its work. In fact, it may thus greatly increase the stores of heat beyond that given off by radiance, and that gained by falling from space; and so assist in forming the store for final dispersal.

The analogies of the other forces in passing through massive media seem to force us to expect such loss causing heat; and there is reason to suppose there is a slight resisting medium to matter in space: the dynamic equivalent of this loss of gravic force, in the life of the particle.

Without a resisting medium to matter in space it is hard to understand how matter can congregate into masses instead of always flying off again with equivalent velocity.

And it is equally hard without a resistance to gravic force in bodies to understand how gravic force can act to cause gravitation, or how the particles in a fixed body, radiating its force, can ever again get energy for dispersal into space.

But the arrangements may be so wonderfully balanced that the one propulsive gravic force may bring particles together from far space to form solid bodies and suns, and then when their course is run, store up energy in them to carry them back into space again, to go onward and build up new systems of glory.

The mountains of the moon, which have been called dead volcanoes, are simply the drop marks of great meteoric masses falling into the light meteoric dust forming in the outer part of the moon. They are similar in their peculiar forms to the rain drop marks familiar to geologists in sand-stones; and have the same peculiar raised rims and concavities, with the bodies which produced them still standing in their centres.

Some seem to have exploded like shells, sending masses in various directions tearing great furrows, some radiating across the whole face of the moon.

They may represent an enormous period, as the absence of wind and moisture would permit marks once made to remain until obliterated in a like way; water, if there, being a frozen dust, and if ever melted sinking into the deep porous mass.

AMERICAN CHEMICAL SOCIETY.

The regular meeting of the American Chemical Society was held Friday evening, May 6, Vice-President Leeds in the chair. Messrs. J. G. Mattison, Theo. Tonnelé, and Dr. Otto Grote were duly elected members, and Messrs. A. H. Van Sinderin and C. P. Sawyer as associate members. Mr. A. F. Hallock was proposed for election. The first paper on the programme was "On a Slight Modification of the Wilkinson Gas Eudiometer," by Mr. James H. Stebbins, Jr., S. B. Having found considerable difficulty in the manipulation of the instrument as described by Dr. Wilkinson, Mr. Stebbins succeeded in overcoming his objections by bringing the stopcock nearer to the Eudiometer itself. It is very difficult, in fact impossible, to properly explain the improvement without illustrations. At the conclusion of Mr. Stebbins' paper, Dr. C. A. Doremus very thoroughly explained the method of procedure used by Dr. Wilkinson in his working of the Eudiometer. This made the matter clearer, still the improvement by Mr. Stebbins was thought desirable.

The next paper * was by Dr. T. O'C. Sloane, "Note on the Purification of Baric Sulphate." The authon finds in order to obtain a precipitate of barium sulphate that will not run through the filter, a few rules must be observed. These he gave as follows: 1st. The solutiou must be barely acid. This end he secures by using cochineal, finding by its use that the neutralization can be more expeditiously and exactly performed than with litmus. 2d. The precipitant is added when the solution is almost up to a boil and kept at that temperature for some minutes. By following these two suggestions a heavy precipitate with a perfectly clear supernatent liquid will be obtained. In case any iron salts have been carried down with the barium sulphate, the precipitate is to be treated first with hydrochloric acid and secondly with sulphuric acid, but this process is open to some objections. It is therefore best to fuse the precipitate with sodium carbonate and a very little sodium nitrate and redetermine the sulphur. As an improvement, Dr. Sloane finds the following method quick and reliable: The sulphur is precipitated in the conventional manner with the previous mentioned precautions carefully observed. The solution is then decanted to the last possible drop through a filter paper; 5 or 10 c. c. of conc. hydrochloric acid are then added and the beaker held in the hand over a hot plate until the acid is brought to a full boil. It is allowed to continue so for a few minutes, then cooled and diluted. The liquid neutralized with cochineal solution, re-acidified and poured into the filter. By this manner a white and clean precipitate was obtained. Dr. Sloane immediately followed with a description of a new "Qualitative Test for Carbon Disulphide and Carbon Dioxide in Coal Gas." A piece of caustic potash, a few m. m. long, is added to ten or twenty c. c. of alcohol, into which a piece of potassium carbonate has been added. The alcoholic solution of potash is placed in a suitable absorption tube and a cubic foot or more of gas passed through it. It is then removed from the absorption apparatus and poured into a test tube. If the gas contains any carbon dioxide, an oily looking layer, nearly colorless, of a solution of potassium carbonate will underlay the alcohol, which latter will have acquired a reddish color. The alcoholic solution, which, if any carbon bisulphide be present, will contain potassium xanthate, is boiled and tested for hydrogen sulphide. Another method, is to add an excess of a copper salt, filter out the precipitated copper compounds and pour ammonia through the filter paper, when a highly characteristic yellow precipitate of copper xanthate will remain behind.

The fourth paper was by A. R. Leeds, Ph. D. Its title was "Upon the Direct Conversion of the Aromatic Amides into their corresponding Azo-compounds."

This paper was a sketch of the recent work which Dr. Leeds has been prosecuting in his laboratory at the Steven's Institute. It consisted, as described in the title, of the details incidental to the conversion of the different aromatic amides into the corresponding azo-compounds with the peculiarities of each commented on. Many of the hydroxylated compounds were also operated on by Dr. Leeds.

Mr. A. A. Julien followed with a very interesting paper "On the Chemical Contents of the Fluid Cavities of Minerals." Mr. Julien is the well known lithologist of the School of Mines in this city, and has a higher reputation in this specialty than almost any other scientist in this country.

He first gave a general outline of the history of the subject. It is only comparatively recent that any attention has been paid to these cavities, which are very minute in size and generally of a rounded shape, though sometimes following the outlines of a crystal, that is to say, the cavity is of the same shape as a crystal of the substance in which the cavity occurs. New York is, for many reasons, the best place to study this subject; for instance, a greater number of specimens find their way to this city. Among the substances found in these cavities are: water, carbon dioxide, nitrogen, sulphur dioxide, ammonia, fluorine, chlorine, oxygen, hydrogen disulphide, and rarely bituminous and light hydro-carbons.

Herkimer, N. Y., is a locality where the latter are frequently found.

Carbon dioxide is, however, the most interesting of these substances to the chemist, and it is also the one most frequently met with. It is found in some fifteen localities throughout the United States. One locality is known in New York State. These cavities are generally found in granites, granito-porphyries, hornblendic and other gneisses and in smoky quartz. The most characteristic feature of the carbon dioxide in the cavities is its remarkable expansive quality—so great that in touching it the warmth of the hand will completely vaporize the liquid compound. Mr. Julien has devoted special attention to the determination of the temperature at which the liquid expands and for that purpose has devised a form of apparatus to be used in such estimations. The piece of mineral containing the cavity is mounted on a microscopic slide and placed in the new apparatus, which consists of a long metallic box, with a small tube on the surface, to which a rubber tube is attached. The whole apparatus is placed in the microscope. On blowing into the rubber tube sufficient heat is obtained to cause the expansion of the bubble of carbon dioxide. Readings are made of the temperature at which the bubble disappears and also of the temperature at which the bubble reappears. Mr. Julien's results agree within two tenths of a degree Fahrenheit.

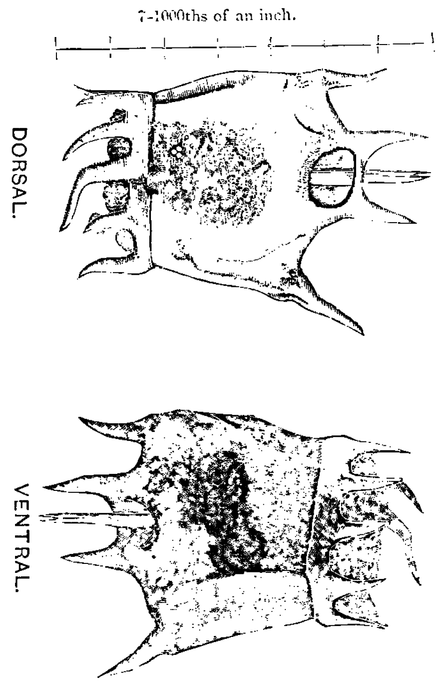
Thus by the ordinary method (Fuessi's) two results, 80.1 and 79.5 were obtained, while with the improvement six results were obtained as follows: 79.6, 79.4, 79.6, 79.5, 79.5 and 79.6. Mr. Julien also gave a very interesting description of "Reticular Fluid Cavity" in Topaz from Brazil, whose bubble was the largest ever discovered, being 2.28 m. m. in length. He also referred to the spontaneous motion observed in the bubbles and to the general bearing of the entire subject of the gensis and formation of rocks. M. B.

* I would acknowledge my indebtedness to Dr. Sloane for his kindness in lending me his original MSS.—M. B.

M. DUCHEMIN, the inventor of the compass with circular magnets, now adopted in the French navy, has lately devised, for correction of compasses, a system of magnetic compensators, in which magnetic bars of annular or circular form are used in place of the straight ones. These have the advantage of insuring much greater magnetic stability than straight bars, especially when lightning occurs in the neighborhood of the ship.

A NEW ROTIFER.

In a filtering of Hemlock lake water (Rochester's water supply) made in August of last year, I noticed a rotifer that at once struck me as different from any that I had before observed or seen described. On classification it proved to be a Brachionus, and a diligent search through the somewhat scattered literature on the subject has since failed to satisfy me that this form has ever been described.



BRACHIONOUS CONIUM.

The Micrographic Dictionary uses the classification of Ehrenberg, while Carpenter in his work, " The Microscope and its Revelations," adopts that of Dujardin. While all classifications of the Rotatoria thus far made are in some ways unsatisfactory, that of Ehrenberg seems the least faulty, and according to it I find that this organism, by reason of having its rotary disk divided into two parts (*Zygotrocha*) and having a carapace, would show that it belongs to the family "*Brachionæa.*" There are five genera in this family. The Brachionus has one eye-spot and forked foot, and to this genus the rotifer unquestionbly belongs: "*Brachionus Conium.*"

Lorica irregularly truncate, slightly reticulated over entire surface except the collar carrying frontal spines; this latter portion has a hard vitreous appearance.

Ten frontal spines, the middle one on the dorsal surface longer than the balance and describing almost a right angle turn near its center to one side. This spine half as long as the carapace of the rotifer. Eye-spot prominent. No openings on dorsal surface of carapace.

Four posterior spines, one at either extreme side and one on either side of anal opening. Tail or foot, slender and bifid. Extreme length of rotifer including anterior and posterior spines, seven one-thousandths (7-1000ths) of an inch.

Unfortunately a dead specimen had to be used for the drawing, hence no definite description can be given of mouth parts for internal structure. The external appearance is, however, so strikingly characteristic as to serve all purposes of identification until the internal structure can be fully described. H. F. ATWOOD.

MICROSCOPICAL NOTES.

The subject of standard screw gauges was recently brought before the R. M. S., and the question of the accuracy of 50 duplicates, made for distribution, was discussed. Mr. Bevington considered " they were as near the standard as could be expected." Mr. Beck pronounced them on trial to be defective. It seemed to be conceded that the original "taps and dies" had been lost, but as Mr. Crouch thought that the present set of duplicates was sufficiently perfect for all practical purposes, we suppose opticians must rest and be thankful for what they can obtain. Considering the deterioration, which must occur from the wear and tear of the cutter, it is to be regretted that perfect accuracy cannot be given to the standard gauges issued by this society.

On the presentation of a paper by Mr. Shrubsole on the "*Diatoms of the London Clay,*" the President, Professor Martin Duncan, made the following interesting statement on the subject. He said that "those who studied this class of subjects would be greatly interested in the paper which had been brought before them; and no doubt had it been read before the Geological Society, there would have been considerable discussion upon it. The London clay had at the bottom of it large beds of pebbles; these were all water-worn, and clearly indicated an old shore. Just above this, on a sinking shore like it, would be precisely where they should exexpect to find diatoms. But the London clay just above this became a little more marine, and this fact would acccunt for their not finding these fresh-water forms there also. Then it should be remembered that the occurrence of diatoms was subject to great variations, and that they were always found in greatest abundance in the neighborhood of silicious rocks. As regarded their age, he thought there could be no doubt that they lived at the time of the Lower Eocene. There were, however, some peculiarities about the London clay, there being no other strata which were deposited under the same conditions, because it was not a reef deposit, but it positively told the story of an open estuary leading down to a very large river. This was one reason why they would not find the diatoms in similar deposits in Italy or Wales. It was not an uncommon thing to find that in other fossils the carbonate of lime was replaced by sulphide of iron. Phosphate of lime was often also replaced by sulphide of iron, and the interstices of other fossils were often found filled with the same substance, which was an exceedingly common mineral in the London clay. Silica was not the difficultly-soluble substance which it was formerly thought to be, so that its place could be as easily filled up by any other mineral which was less soluble than itself—from which consideration he thought the matter might be explained. But when they came to the question of antiquity, it was not so easy to give an opinion as to whether Count Castracane's diatoms in the Carboniferous series were with good reason thought to be diatoms. In the Tertiary of course they found them; but if Count Castracane's propositions hold good, we ought to be sure to find them in the intermediate series."

Mr. Shrubsole said Mr. Kitton's idea was that they were fresh water diatoms which had been washed down into the coal-beds.

The President expressed himself unable to accept such a suggestion.

LAST week the Whittaker Court Marshal was continued, and Dr. Piper of Chicago, was examined as an expert on Microscopy. In cross-examination questions were submitted to the witness on the construction of the Microscope, which Dr. Piper admitted were beyond his knowledge. One question related to the composition of the glass used for the construction of lenses for the Microscope.

Possibly few Microscopical experts could answer

questions on this point, and we would be obliged if any of our correspondents would furnish a few facts. We understand that some makers make use of a very soft glass, the surface of which becomes defaced in a short time: a dealer calls this "*spongy glass.*" We would like to know where the respective makers purchase glass for objectives, and its composition.

ASTRONOMY.

COMET (*b*) 1881.

Prof. Barnard, of Nashville, Tenn., announces the discovery of a comet on the morning of May 12, 1881, in R. A. 22^h 59^m 18^s, dec. + $14°$ $24'$ $30''$. An observation on the following day gave R. A. 22^h 58^m 52^s, dec. + $14°$ $36'$ $0''$, thus indicating a motion 24^s in R. A., and $11''$ in Dec. The comet is reported as very faint.

ON THE USE OF THE ELECTRIC TELEGRAPH DURING TOTAL SOLAR ECLIPSES.*

If we suppose a single observer to be prepared for the observation of all the total solar eclipses of a century, we shall find that the entire amount of time during which he may contemplate the totally eclipsed sun will not differ much from an hour. We may be sure then of the expediency of any scheme whereby the rare moments of these eclipses may be utilized to their utmost extent. If such scheme is devised, two important results are likely to follow.

(1.) Economy of the sum-total of energy in any particular line of solar research.

(2.) A consequent enlargement of the means of research in other directions.

The general conception of the scheme proposed in this paper may be very briefly stated: Suppose a station to the east and a station to the west on the line of any total eclipse, as widely separated as practicable, and equipped for similar observations of discovery during the progress of the eclipse; the method proposes the electro-telegraphic transmission of important observations made at the western station to observers at the eastern station, with due speed for their verification or rejection when the lunar shadow reaches the latter station.

For illustration, consider the next total eclipse,—that of 1882, May 16. In detail, the particular advantages in connection with this eclipse seem to be about these:

(1.) The path of totality is almost exclusively on land. Central eclipse begins in West Africa; the line of totality passes to the north-east, crossing Upper Egypt and the Nile at El-Akhmym; thence over the Red Sea, crossing the Tigris a few miles to the south of Bagdad; then passing a little to the south of Teheran, it traverses Central Asia, and leaves the Asiatic Continent somewhat to the north of Shanghai.

(2.) Though not generally through the inhabited regions of the globe, the path of totality lies through several inhabited regions which are widely separate, viz: Egypt near the Nile, Central Persia and Eastern China.

(3.) These regions are inter-connected by telegraphic cables and land-lines.

Now, we will suppose that an important observation of discovery is made at El-Akhmym,—an observation of an intra-mercurial planet for example. Between 40 and 45 minutes of absolute time elapse before totality comes on at Teheran. During this interval the observer at El-Akhmym will have an abundance of time for transcribing the apparent magnitude and the precise position of the new body, and transmitting the same to his fellow-observer at Teheran several minutes before the lunar shadow reaches him. The latter observer will then have leisure to proceed with the setting of his circles, the verification of their readings, and the pointing of his instrument to the precise part of the heavens indicated. He may then be able to see the suspected object before the eclipse becomes total. He may also decide upon a neighboring star for comparison with the planet, and thus obtain a very accurate determination of its position. The observer at Teheran should also be prepared for an independent search for the suspected planet, in the event of receiving a negative message from the observer at El-Akhmym.

The observation at El-Akhmym should also be transmitted to Shanghai, (reached by the shadow more than two hours after totality at Teheran), for independent verification at that point. We might thus observe the result of nearly three hours' motion of the planet,—which we might reasonably expect to give important data in regard to its orbit about the sun. Of course, the result of observation at Teheran would also be transmitted to the observer at Shanghai.

It was my intention primarily to have considered the total eclipse of 1882 merely as an illustration of the method proposed. Further investigation, however, seems to show that it is at least one of the two most favorable eclipses during the present decade, if not during the present century.

WASHINGTON, *May* 18, 1881. W. C. W.

*Abstract of a paper by D. P. Todd, M. A., presented before the American Academy of Arts and Sciences, Jan. 12, 1881.

COMET (*b*), 1881, BARNARD.

To the Editor of "SCIENCE:"

On the morning of the 12th, while sweeping the eastern sky in search of comets, at about three o'clock, an object entered the field of my telescope which I strongly suspected was a comet, as I did not know of any nebula near its place. I at once secured its position relative to *α Pegasi*, it being in the field with that star. Its position at seven minutes past three o'clock was:

R. A. 22^h 59^m 18^s
Decl. + $14°$ $24'$ $29''$

The object was watched at intervals until about four o'clock, when daylight prevented further observation. During this time no motion was detected. Wishing to confirm the discovery by a second observation, before announcing it, I waited until the following morning, when upon turning my telescope to the point where the object was seen, I found it had disappeared.

No doubt now remained in my mind of its cometary character. I began a search to re-discover it. After sweeping for some time in the immediate neighborhood, I found it again as day-light was whitening the sky. It was very close north following *α Pegasi*. The object was then only visible when the bright star was obscured by a part of the ring suspended in my eye-piece. It followed the star by six seconds and was therefore in R. A. 22^h 58^m 52^s. I estimated the difference of declination between comet and star, and found it to be in north declination $14°$ $36'$. No doubt now remaining that it was a comet I telegraphed its position to Professor Swift, Director of the Warner Observatory at Rochester. On the morning of the 14th I again began a search as soon as the object had risen above the horizon, but it could not be found. At first I attributed my not finding it to its low altitude and the bright moonlight. The search was continued until daylight, and I was deeply mortified at not finding any trace of the object. In the morning telegrams from Rochester and Boston announced failure to find it at those places.

A short search this morning, when the sky had cleared, at about day-light, resulted no better than yesterday morning. The object on the 12th was slightly smaller than Swift's last comet, which I had been observing on the 11th, and was probably a little brighter. On the 13th it

appeared very faint. This I attributed to its proximity to the bright star.

I shall continue the search for it. The moon will leave in a few days and I then hope to be able to see the comet again. E. E. BARNARD.

May 15, 1881.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE :"

I should have attempted a reply to the restrictions of Mr. Dopp before this time if I had not had my hands too full of other work, but lest any might think I have nothing to say if an answer of some kind did not shortly appear, I will ask the favor of a little space, and first I entirely disclaim the pretension of undertaking to reconstruct Physical Science which Mr. Dopp seems to impute to me, and whatever was put forward as new was only hypothetical, and perhaps I was not guarded enough in specifying it as such. Yet there is more that may be said for some of the statements made than appears in those papers, which were very brief and did not pretend to give references. But now if I shall deal with the subject of internal and external energy which is attacked in the last part of Mr. Dopp's paper, it will save saying very much about the first part.

Mr. Dopp quotes from Maxwell's works on Heat, and says they disprove and invalidate all my calculations. But it will probably be allowed to hear Maxwell in 1875 against Maxwell of 1872 :

"In 1860 I investigated the ratio of the two parts of the energy on the hypothesis that the molecules are elastic bodies of invariable form. I found, to my great surprise, that whatever be the shape of the molecules, provided they are not perfectly smooth and spherical, the ratio of two parts of the energy must be always the same, *the two parts being in fact equal.*" He also says a few lines beyond when speaking of the researches of Boltzmann, he "makes the whole energy of motion twice the energy of translation." See Nature, volume 11, p. 375. This language justifies my work and my calculations are not invalidated. What is to be understood by $E' - E = \epsilon$, is their difference and not a ratio, and the expression in the paper is wrong, but there is nothing in the paper that depends for its correctness upon any mathematical expression in it, whether it is right or wrong, and cannot be raised against it. That is to say, there is nothing in the first paper that is a deduction from any mathematical work given.

As to my definition of ether as not matter, again Maxwell is quoted against me, and I will therefore again quote Maxwell in my favor. "According to Thomson, though the primitive fluid is the only true matter, yet *that which we call matter is not the primitive fluid itself but a mode of motion of that primitive fluid.*" See Art. *Atom* Enc. Brit., 9th Ed. The italics are mine, but if it does not plainly make a distinction between ether and what we call matter, then I don't understand it. But I claim more, that to call ether the primitive matter is to call two different things by the same name, and my first paper was a protest against that. Newton's law of Universal Gravitation states that "every particle of matter in the Universe attracts every other particle of matter," and until it is discovered that ether possesses this property of attraction, I hold that the name *matter* should not be applied to it. If, however, any one thinks it to be a proper use of words, I shall not quarrel with him, only when he talks to me of matter I shall need to ask whether he means gravitative matter or non-gravitating matter. As for the objection that I use the term density applied to ether and am therefore to be held to what is implied in the word ; any one who undertakes to express a new conception must either employ words that have some fixed meaning or else coin some new word which in its turn must be defined with old words. So while the term density conveys my meaning in a tolerable way, I do not wish to have it imply that density in ether and density in matter are identical. In the same article on Atoms, Maxwell says concerning the vortex-ring theory : "We have to explain the inertia of what is only a mode of motion," and this is in strict accordance with all I have written about it.

We do know that the motions of atoms set up corresponding motions in the ether, and it is not difficult to perceive how it may happen, though the particular mechanical conditions may not all be known. *Assuming that the conditions are mechanical*, then the analogy of the vibrating tuning fork is not so far fetched as it might be. I do not see the necessity for my being held to atoms combining in only one plane. It is as easy to see that three or four or more could all unite at the same place so as to form a radial structure or a triangular one when one of the two represented in the diagram should swing round 120°, which, so far as I can see, would not imperil its stability at all, and it then would be in position for another similar atom to unite with each, and so on almost any kind of a geometrical solid made. But I did not intend to assert at all that in this hypothesis there was anything more than an idea. I am not ignorant of the molecular form of ordinary matter, but my assumption was that the molecular form was due to its vibratory energy, and, consequently, I was mostly treating of atoms, and the statement was made that at or near absolute zero the chemical affinity was *nil*, and hence dissociation. This is plainly the case if chemism is due to heat vibrations, but it is corroborated by mathematical calculations. In a paper read before the American Academy, in February last, by Mr. D. E. N. Hodges, of Harvard College, but which has not yet been published, the same conclusion is deduced from thermo-dynamic considerations, namely, that at absolute zero "there can be no cohesion of molecules, and probably the same for atoms ; it is the temperature of dissociation." Mr. Dopp quotes from Professor Tait what he knew about the phenomena of vortex-rings, but since Mr. Dopp's paper was written he has probably heard of some more phenomena of vortex-rings. See "SCIENCE," April 16th.

As to the paper on Atoms as forms of Energy the *idea* is not mine, but Thomson's, and whether or not the method therein shown of computing atomic weights is mathematical jugglery, as Mr. Dopp calls it, all I have to say is, I did not stake anything upon it. I thought if matter is a form of energy, the fact should appear in atomic weights, and so I made the calculations and published them, and if anyone thinks they signify nothing, why I will not quarrel with him. After so long a paper finding fault with anything I had written, it was something of a pleasure to read that he thinks my theory can be made "a fair working hypothesis to explain adhesion, cohesion, and even crystallization,—surface tension of liquids and capillary attraction, and possibly those of osmosis, dialysis and occlusion."

This is not an unworthy stock of phenomena to explain, and if what I advanced can not be made to do all I proposed to have it do, I might be content if it explained in a fair way any one of the above phenomena.

A. E. DOLBEAR.

COLLEGE HILL, Mass., *May* 10th, 1881.

To the Editor of SCIENCE :—

As two of your correspondents, Mr. A. E. Dolbear and Mr. George W. Rachel, have adversely criticized certain points in my article in the April 9 number of "SCIENCE, and as I still consider my position as stable, I must request a limited space to reply to these gentlemen.

The main difficulty seems to be that I have gone

counter to certain authors whom they are disposed to consider as authorities. But, in my view of the case, Science has no authority, except the authority of facts, and theoretical views are always fair food for criticism. Mr. Dolbear quotes from Clausius to the effect that "all heat existing in a body is appreciable by the touch or the thermometer; the heat which disappears * * * * exists no longer as heat, but has been converted into work." Heat is undoubtedly appreciable, but not necessarily measurable, by the touch or the thermometer. As the heat capacity of any substance increases its temperature effect for equal volumes of inflowing heat diminishes, so that the thermometer fails to indicate the exact quantity of heat which a substance receives in passing through a fixed range of temperature. It is customary in late authors to speak of this apparently lost heat as converted into work, or, in other cases, to speak of it as changed from actual into potential energy. This is, undoubtedly, a very convenient way of getting around the difficulty; but, with all due deference to the distinguished writers who advance this hypothesis, I venture to question if it is a strictly scientific way. To come plump up against a difficulty in your path, to explain this difficulty by a nicely sounding word which explains nothing, and then to go swimmingly on, enables one to get over a great deal of ground in a short time; but it is very apt to leave stumbling blocks for those who come after.

I should certainly like to see a precise definition of the word "work" in this connection. Heat produces a certain effect. That effect is called work. But the important question remains, what has become of the heat? It was a motion. Has it ceased to be a motion? If so, then motion can cease to exist. Yet I hardly think any scientist will admit such a possibility. But if it has not ceased to be motion, where is it? Is the word "work" advanced as a name for some new mode of motion? Whether it is or not, however, it fails to explain what has become of the heat. We meet with a like difficulty in the theory of the conversion of actual into potential energy. Actual energy we can readily comprehend; it is the energy of the motion of masses. But what is potential energy? It is a possibility of mass motion. A body rests upon the earth. It cannot possibly descend further. It has no potential energy. A body is suspended in the air. It may possibly descend further. It has potential energy. Potential energy then, is possibility of motion. Actual motion has been converted into possible motion. If this amounts to more than the explaining of a difficulty by a meaningless phrase, I should certainly be glad to have some one scientifically explain the explanation. I must quote from my former article: "Motion is motion and cannot possibly be or become anything else." Actual motive energy cannot cease to exist, and be replaced by an abstract possibility of motion, called potential energy.

In regard to Mr. Rachel's remarks on my views respecting variation in heat capacity, he must permit me to correct his quotation. He quotes me as saying: "Temperature and heat are very different things." I find my expression to be: "Temperature and *absolute* heat are very different things." There is a considerable difference of meaning between these two expressions, which it would have been well for him to give me credit for. The main difficulty in the minds of both my critics seems to be a somewhat confused idea as to what constitutes heat. Mr. Dolbear claims that the free vibration of molecules is not heat. In this he certainly disagrees with most authors. Mr. Rachel states that "latent heat is not heat." He intimates that it is work, but will he be kind enough to explain scientifically just what work means in this connection? He says further, "Water does not contain more heat than ice at 32°; it contains * * * more motion, but not motion of the heat kind." Of what kind then? "Nor is it true that as density diminishes the heat capacity increases." The heat has disappeared as heat, "but it nevertheless exists in the gas as a greater range of mobility."

We here get his definition of "work." It is "motion, but not motion of the heat kind;" it is "a greater range of mobility." Motion, then has not ceased to exist, and we have been splitting hairs about nothing. It is molecular motion, but not the special mode of motion which he calls heat. Yet it would be well to bear in mind that scientists are somewhat indefinite in their ideas as to just what mode of motion does constitute heat. In one case they speak of radiant waves as heat, in another as local molecular vibrations as heat; in a third, of the free motions of gas particles as heat, and in a fourth, of motive influences which cease to affect the thermometer as heat, for what else is meant by absolute heat? The *authorities* certainly consider that heat continues to exist as heat in the case of increased heat capacity, when they assert that specific heat varies with variation in the temperature of substances. Thus it seems that all motive influences of which we become aware in matter, outside of gravity, electricity, magnetism, light, chemism, and mass motion, are grouped together as heat, their varying conditions being simply pointed out by qualifying adjectives. The phrase, "Latent Heat," has by no means gone out of use. Sir William Thomson, in the last edition of the Encyclopedia Britannica, considers it necessary to still retain it. In fact there are various modes of motion, some centrifugal, others centripetal in their effects, so closely related to ordinary heat, that it has proved more convenient to consider them as special heat conditions than to devise separate names for them.

Mr. Rachel is still more decisive in regard to another portion of my article. He says, "Mr. Morris's conception of the action of gravity is still more erroneous. This gentleman says, 'the earth must fall towards the body with the same energy that the body displays in falling towards the earth'! Now the two fundamental laws of gravitation, as discovered by Newton, are attraction acts in direct proportion to mass and in indirect proportion to square of distance. The statement of Mr. Morris is therefore absolutely false."

Perhaps so, yet I hardly think that Newton himself would have so absolutely denied my proposition. Let us suppose the falling body to be increased until it equals the earth in weight. What would follow then—would not gravity cause them to approach each other with equal energy? Their attractive pulls upon each other would be equal, and therefore the effects of these pulls must be equal.

If, however, the falling body be greatly decreased in weight, this may seem to some to change the elements of the problem. Yet it can readily be shown that difference in weight makes no difference whatever in the result. We must not look upon the earth as fixed and the falling body alone as movable. They are both freely floating masses, each capable of yielding to any exterior impulse. The size or weight has nothing to do with the question. If an atom and the earth be side by side, and be attracted by a distant mass with the same vigor, they must move with equal energy towards it. Yet an energy which would give the atom excessive speed would produce an inappreciable effect upon the earth.

Suppose, for the sake of illustration, that the falling body weighs one pound and the earth one million pounds. Then the falling body will attract each pound of the earth's mass with a vigor dependent on its distance, and be attracted by it with equal vigor. To reach the whole attraction of the falling body we must add together this million of separate attractions. But, in like manner, to get the whole attraction of the earth we must add together its million of separate attractions. The body exerts a separate attraction upon each pound of the

earth's mass. Each pound of the earth's mass reacts with an equal vigor of attraction upon the body. We must add all these separate attractions together to get the whole sum of attraction in either case, and these whole sums are necessarily equal. The body, therefore, attracts the earth with as much vigor as the earth attracts the body, and necessarily, therefore, they must approach each other with equal energy. Of course not with equal speed. Under the above supposition their respective weights were as a million to one, and a million pounds falling one inch would be equivalant to one pound falling a million inches. Their acceleration in speed must likewise, in both earth and body, obey the law of gravitative acceleration.

It is well, therefore, to bear strictly in mind, that in gravitation, as in every other form of force, action and reaction are always equal and opposite.*

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PRIMEVAL ROTATION AND COSMICAL RINGS.

II.

To the Editor of "SCIENCE":—

Prof. A. Winchell, recounting the history of formation of the solar system from a sphere of incandescent gas, says: "The cooling and contraction of this vapor inaugurated a rotation." [1]

Matter is governed by law, hence the ball of gas must have obeyed laws governing gases. Men have detected several laws of nature, while doubtless there are others eluding research. The globe of gas was dominated by known or unknown laws; if by unknown, no scheme of planetary evolution can be outlined; by known, hypotheses are tested by their application. There exists a doctrine, the Nebular Hypothesis, and we take it for granted that its advocates conceive the gaseous sphere to have been wrought by known laws.

But no law of nature yet discovered is able to cause a sphere of gas to rotate.

Contracting by cooling did not begin rotation, for, by dynamic laws, the mass was not hot, but cold. If hot, contracting would not cause rotary motion, but would give rise to two motions, centripetal and peripheral, both radial instead of circular. The heaviest atoms, gravitating towards the centre, would displace the lightest towards the circumference.

Repulsion did not exist; this force can only obtain in matter *not* dissociated. Repulsion causes dissociation and vanishes, gravity reasserting dominion. Hence, repulsion is more ancient than that gravity which caused the mass to develop a solar system; else the first state of matter was in dissociation.

These things are unknowable; therefore, with adherents of the hypothesis, we dismiss repulsion, leaving the mass subject to no known energy but gravity. If repulsion did act it could not cause rotation. Gravity could never cause the ball to turn; it would bring every atom to a rest. The whole mass would arrange itself in concentric strata, whose distance from the centre would depend on specific gravity. Calm would ensue unless pressure was sufficient to force atoms within range of chemism. Chemical reaction would have no power to start axial revolution. It would evolve heat, repulsion and temporary expansion, which, waning, would leave the mass smaller through combination, no sign of rotary motion having appeared. The mass extended half way to α Centauri, it being equal in mass to the sun. Helmholtz has shown that if the matter in the solar system expanded to Neptune, "it would require several cubic miles to weigh a single grain."[2] But the same matter filled a sphere whose radii were half the distance of the stars in length. Estimation of its tenuity indicates that a space as large as the moon only contained a grain. Yet it was "intensely heated."[3] It is not known how many atoms make a grain; counting them by the million, they were yards apart, in frigid voids—hot! Obeying gravity, they descended with slowest conceivable motion; at no point in their fall displaying tendency to move in arcs of circles at right angles to their radial movement, which they must do to begin rotation in the cosmic sphere. In the present state of knowledge, judging from laws at work in the Universe, it can be safely asserted that the ball had no rotary motion. Ignoring these considerations, we will assume with Winchell that it was in revolution.

"The cooling and contraction of this vapor inaugurated a rotation which was inevitably accelerated to such an extent that a peripheral ring was detached which became a planet. The same process continued and other rings were detached which became other planets in due succession. Similarly, the planetary masses detached rings which became their satellites."[4]

Conceiving the mass to have cut loose from 61 Cygni and other cosmic masses; admitting cooling, contraction and acceleration, then the sphere would be unable to cast off by any law of nature hitherto discovered, the least particle, to say nothing of a massive ring. The ball had dwindled to the orbit of Neptune, acquiring such volocity as to no longer remain intact, so it cast off equatorial matter enough to form that planet.

The rate of motion on the equator was only 3.36 miles a second; and a vacuum as made by Crookes is as a solid compared with the density of the ring; yet Neptune's mass is nearly 102 sextillions of tons[5]. The material, being exterior, was of the lowest specific gravity of any in the mass; thence its volume was enormous; so great as *not* to be peripheral. The word periphery alludes to the surface, and Winchell says the ring was peripheral. It was not,—it was formed of gas torn up from a depth of hundreds of millions of miles, in order to secure substance sufficient to form Neptune. If not,—the mass was piled above the level of the equator, an impossibility, as gravity would bring it down. As soon as force raised a line of atoms above the equatorial level, around the ball, the next line of atoms below would ascend, *then the next, and so on. The poles would depress* causing the mass to assume lenticular form. This would retard rotation, allowing central attraction to regain control, bringing the mass to a sphere as in the beginning. This oscillation must take place so long as the mass remained a gas. Should it become fluid, then the alternations would be between a sphere and spheroid, and the mutation would obtain until solidification sets in. No atom at any period had power to overcome gravity, the stability of the mass being assured by inhering laws. The mass of the solar system, the mass of any planet, the direction and velocity of the planet's original motion, determine what orbit it shall traverse.

The orbit of Neptune is determined; it makes regular revolutions, hence the centre of the assumed ring that formed it, when abandoned coincided with the present track of the planet's centre. Therefore the ring was not detached when the mass was lenticular, for its edge then extended far beyond where Neptune now revolves; if it had been the planet would now describe our orbit much *farther* from the sun.

The mass reached the present path of Neptune when spherical, and that world was thrown off where it now makes circuit, the mass being a sphere when it parted with its first ring.

*In my previous article, above referred to, there is a typographical error, which slightly confuses the meaning. On page 167, line 49, the phrase "this force is increasing," should read, "this force is unceasing."

[1] Geology of the Stars, p. 260.
[2] Youman's Correl. and Con. Forces, p. 231.
[3] Geology of the Stars, p. 279.
[4] Geology of the Stars, p. 279.
[5] Chambers' Astronomy, p. 898.

Then a segment half way to Uranus was torn away entirely around the ball, and the rupture took place along the chord of the arc. A section of this ring would be flat inside and curved outside; else a ring lifted out of the equator quite around the sphere, whose sections were circular, leaving concave walls of gas in north and south latitude. Neptune would condense somewhere on a line in the centre of gravity of the ring. In either case the orbit of the first world would be *nearer* the sun than now. It could not have been thrown off the surface, as there was not material enough; nor from the edge of the lens-shaped mass, nor from beneath the surface of the sphere, for from either place the orbit would not be where it is. It could not have been cast off at all. EDGAR L. LARKIN.

NEW WINDSOR OBS., *May 11th*, 1881.

BOOKS RECEIVED.

THE HUMAN BODY.—An Account of its Structure and Activities and the Conditions of its Healthy Working. By H. NEWELL MARTIN, D.Sc., M.A., M. B. Henry Holt and Company, 1881.

This book is the fourth of the "*American Science Series*" of manuals prepared under the direction of Messrs. Holt and company, and will be found of equal value, as a popular guide to the subject treated, to the three works which preceded it. It is a reliable work, being compiled from the best authorities, and is not intended for specialists, but for general readers and students.

In an age when the Physician is called upon to explain to his patient, the *raisen d'etre* of the treatment suggested, and even to describe the peculiar condition of the organs affected, some knowledge of Human Anatomy and Physiology appears essential to those who desire to act as consulting physician in their own cases.

To meet such a demand for a popular work on the human body, Dr. Martin has prepared the present volume, which is free from technicalities, or scientific terms requiring interpretation. The reader has the advantage of one hundred and sixty-five excellent illustrations, and as Dr. Martin's style of writing is both clear and comprehensive, the task of the reader is an easy one.

The earlier works of this series have been reviewed in "SCIENCE" and comprise the following manuals: *Astronomy*, by Professors Simon Newcomb and Edward S. Holden: *Botany*, by Professor C. E. Bessey: *Zoology*, by Professor A. S. Packard, Jr.

OSTEOLOGY OF SPEOTYTO CUNICULARIA Var. Hypogæa, and of Eremophila alpestris, by R. W. SCHUFELDT, U. S. A.—Extracted from the Bulletin of the U. S. Geological and Geographical Survey—Washington, Feb. 11th, 1881.—Four full page illustrations.

ABSTRACT OF TRANSACTIONS of the Anthropological Society of Washington, D. C., with the annual report of the President.—For the year ending Jan. 20, 1880, and for the second year ending January 18th. 1881. Prepared by J. W. POWELL.—Washington, 1881.

THE TWELFTH ANNUAL REPORT of the American Museum of Natural History—Central Park, New York City—Dated February 15th, 1881.

REPORT of the Cruise of the U. S. Revenue Steamer *Corwin* in the Arctic Ocean, by Capt. C. L. HOOPER, U. S. R. M,—November 1, 1880—Washington, 1881.

REPORT of the Director of the Detroit Observatory of the University of Michigan—October 1, 1879, to January 1, 1881, Ann Arbor, Michigan, 1881.

ABSTRACT of some Paleontological Studies of the Life History of Spirifer lævis H. by, Professor H. S. WILLIAMS of Cornell University, Ithaca, N. Y.—Reprinted for American Journal Science.

OBSERVATIONS on Jupiter by L. TROUVELOT—Presented March 9th, 1881.—Reprinted from the proceedings of the American Academy of Arts and Sciences.

PROCEEDINGS of the U. S. National Museum, 1881. Check List of Duplicates of Fishes from the Pacific coast of North America (221 Species) distributed by the Smithsonian Institution in behalf of the United States National Museum.—Prepared by DAVID S. JORDAN and PIERRE L. JOUY.—April 13, 1881.

DESCRIPTION of a new species of Squalius (Squalius aliciæ from Utah Lake, by PIERRE LOUIS JOUY.

DESCRIPTION of a new Gobioid Fish (Othonops eos) from San Diego, Cal. by ROSA SMITH.

ON a Duck new to the North American Fauna, by ROBERT RIDGWAY.

ON Amazilia yucatanensis (Cabot) and A. cerviniventris, Gould, by ROBERT RIDGWAY.

DESCRIPTIONS of new species of Fishes (Uranidea marginata, Potamocottus Bendirei) and of Myctophum crenulare, J. and G.—by TARLETON H. BEAN.

NOTES on the Fishes of the Pacific Coast of the United States by DAVID S. JORDAN and CHARLES H. GILBERT.

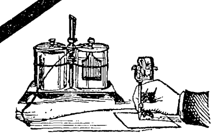
In this paper descriptions are given of 109 species of fishes known to occur along our Pacific Coast between the Mexican boundary and that of British Columbia, with notes on the distribution, habits, size, value, etc., of each species, in advance of the publication of a general descriptive work.

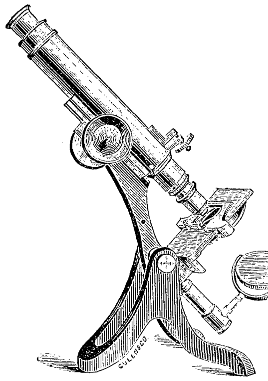
AMERICAN KINDERGARTEN MAGAZINE.—Edited by Emily M. Coe, Bible House, New York.

We have pleasure in recognizing the sterling merit of this excellent little Monthly, a leading feature of which appears to be an attempt to popularize science in a form suitable for children. The present number contains articles introducing the young readers to the best methods of classification of the Animal Kingdom. The journal is in its third volume, and is sold for one dollar a year.

NEW APPLICATION OF THE SUB-PRODUCTS OF COAL-TAR.—Mr. Sanders, of St. Petersburg, has succeeded in producing from the heavy oils of coal-tar, a new substance which, in many cases, takes the place of india-rubber with advantage. It is prepared in the following manner. A given weight of a mixture in equal parts of wood-oil and coal-tar oil, or of coal-tar and hemp-oil, is heated for several hours, at a temperature of about 318° Fahr., so as to disengage the injurious substances and increase the viscosity of the mass, until it may be drawn out in threads. A second quantity, equal to the former, of linseed-oil, preferably thickened by boiling, is now added, and also from one-twentieth to one-tenth per cent. of ozocerite with a little spermaceti. In the meanwhile, the mass is kept at a uniformly high temperature for some hours, when from one-fifth to one-half part of sulphur per cent. is added, after which the product is moulded or otherwise worked in the same manner as india-rubber. The proportions of the three oils named above may be varied so as to obtain a harder or more elastic substance, as may be required. The product is elastic and tenacious, standing the weather better than india-rubber, and is not deteriorated by great pressure or a high temperature. It is said to be specially suitable for the insulation of telegraph wires, and may be employed alone or mixed with india-rubber or similar resinous substances.

ELECTROLYSIS.—Mr. E. F. Smith finds that a black hydrated oxide, $Ur_3\ O_4$, is precipitated when a galvanic current is passed through a solution of uranium acetate, formate, or nitrate.





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The American Museum of Natural History, Central Park, New York, (Edit.); The Storage of Electricity; Sensibility and its Diverse Forms, Translated from the French of M. Oltramare by the Marchioness Clara Lanza; Contribution toward a New Cosmic Hypothesis, by Samuel J. Wallace; American Chemical Society; A New Rotifer, by H. T. Atwood; The Discrepancies in Recent Science (Correspondence); Primeval Rotation and Cosmical Rings, II., by Edward L. Larkin; Books Received; Notes, Astronomical, Chemical, Microscopical, &c., &c., &c.

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